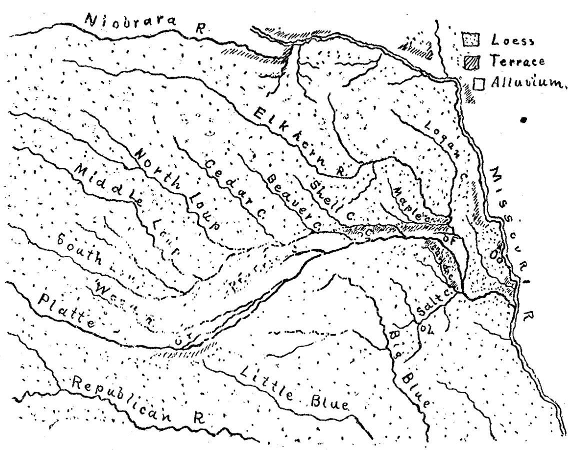
same east-north-east direction, and are clearly analogous with the lower course of the Loup, where it connects its various branches. It seems not very improbable that the channel mentioned by Professor Hicks as connecting the South Loup with Wood River may be of the same sort.

3. The hills north of this compound channel, as it might be called, running parallel with the Platte, are of similar height and structure to those south of the Platte, but the hills south of the same channel are more than 100 feet lower, and of different structure. Both are capped with yellow loam of almost the same texture, but underneath the former have a well defined stratum of northern drift east of the meridian of Columbus, while the latter have but a faint trace of it mixed with deep stratified sand. These lower hills, moreover, are less eroded, and are evidently an alluvian terrace formed since the deposition of the older drift and the Loess. This terrace is seventy to ninety feet above the Platte, east of Columbus, and is more sandy and lower further west. The ancient north bank approaches the present Platte again, near Josselyn.

4. Corresponding in level to this high terrace, is an old channel crossing Saunders County along the valley of Sand Creek and in direct line with the upper course of the Maple. East of this is an area of higher land between it and the Platte, which has been recognized as an "ancient island." It may be added, also, that this high terrace seems to be easily correlated with a terrace of



Drainage Map of Eastern Nebraska.

similar height and structure, found at several points along the Missouri, which may be referred either to the "Second Glacial Epoch" or to the time of the second cluster of moraines of that epoch.

The subjoined map exhibits most of the points mentioned above, as well as some knowledge of the drainage, and indirectly of the topography of the surrounding region.

These facts point strongly to the efficiency of the second influence mentioned by Professor Hicks, viz., "Pliocene channel filling," as the principal and sufficient reason for the peculiar arrangement of the Loup channels, rather than a secondary influence. This has been already pointed out by Professor Davis. The Loups did formerly flow through to the Platte, but at a time when it or a portion of it occupied the north channel already described, and when it was flowing on a level seventy-five to a hundred feet higher, relatively, than at present, somewhat as it now occupies the channel north of Grand Island, and probably not long ago occupied a portion of Prairie Creek. The superabundant sediment, the shifting of the Platte to the south in obedience to Ferrel's law,— possibly reinforced by a tipping to the south, — and a deepening of its channel, which may have been partly due to a cutting through of a divide north of the "ancient island" into the lower channel of the Elkhorn, which, again, may have been accelerated by the recent eastward tipping of the region, are sufficient causes to explain the changes of the Platte, Loup, and associated streams since the disappearance of the waters which deposited the loess. The exceptional course of the Platte, however, from Kearney to Fremont, which we conceive was first taken about that time, remains unexplained. The causes which may be surmised are the following: 1. The position of a depression in the bottom of the Pliocene or Pleistocene lake, which may in some way have been produced by unequal deposition of its sediment, or the earlier unequal erosion or deposition of the subjacent formations whose strike here is approximately north-east. 2. A slight fold in the plains a little south of this course of the Platte. Of such no distinct trace has yet been found. There is a slight anticlinal axis crossing the Big Blue near Milford, but it is probably quite limited in extent. 3. This course may perhaps be a survival of a time when this region was tipped toward the north-east, because of the burden of ice which then rested upon Iowa, Minnesota, and eastern Dakota. This is but a conjecture, against which several objections arise, which it is needless to express.

In this connection, it may be helpful to call attention to a similar bend in the Arkansas in central Kansas, and to note that in each case the exceptional direction is upon more recent beds near, and parallel to their junction with, the upper Carboniferous. This may be a straw which would indicate that our first surmise may have some truth in it.

Concerning the efficiency of abstraction to change lines of superficial drainage, we may find considerable light from the study of this region. The remark of Professor Davis, that this rarely occurs where formations are nearly horizontal, seems well supported. Such is the slope of the country, and the porosity of the deposits, that the headwaters of the Big Blue rise a little below the level of the Platte adjacent, and the tributaries of Salt Creek rise below the level of the Big Blue near by, so that it is possible that water may leave the Platte between Kearney and Columbus, pass into the Blue, be drawn off into Salt Creek, and return to the Platte through the latter stream. And yet I know of no clear case of change of channel by abstraction in the whole region. The abundant sand, through the water flows underground, renders an open channel unnecessary. In fact, it may be argued that abundant sand tends to prevent the formation of superficial streams, unless there be first a velocity of flow sufficient to carry the sand easily, which cannot occur unless the flow is concentrated in some way. This is frequently noted in the sunken rivers of deserts. Possibly this may have had something to do with the exceptional course of the Platte before considered. Dunes form an important part of the divide between the Platte and the Little Blue south of Kearney.

One word further, regarding the comparative slopes of the Loup and Platte, to which Professor Hicks has called attention. Do we not find here examples of the law that declivity varies inversely as the quantity of water, as pointed out by Gilbert in his masterly paper on "Land Sculpture," in his report on the Henry Mountains? Although the Platte is much the more important river, by the time it has reached Kearney it is much reduced by evaporation and abstraction; then, because of its shutting off its tributaries by its abundant sediment, as before noticed, it is so reduced that it is often smaller than the Loup at their junction, even sometimes ceasing to flow above the surface, as I have been informed, while the Loup flows with a good current. On the other hand, the Loup is not so much exposed to evaporation, and has numerous tributaries, which having more frequently cut through the sand stratum, and on the lower side of its sloping basin, are more apt to be fed by springs than lose water by seepage.

J. E. TODD.

Tabor, Iowa, Feb. 29.

Estimates of Distance.

BESIDES the very interesting inferences drawn by Mr. Bostwick from his experiment (*Science*, Feb. 26, p. 118), one or two others should be suggested, in the hope that they may lead to some further investigation.

1. Is not an effect of fatigue shown in the eight or ten per cent by which the average observer's "mean deviation" from his own "average" is increased when the last ten of his thirty estimates are compared with the first ten? Should not this effect be greatest,— perhaps both appearing earliest and increasing most rapidly

with the number of observations made,— when the observer is quite untrained; while good previous mental training in things more or less analogous to those tested by the experiment might enable the observer to utilize promptly the practice being got in the experiment itself, and so might for a time overbalance the effect of fatigue? Thus, in the present case, the deviation increased most with the child A. L. B. and one other person, and decreased most with the artist L. F. and one other, but the data are too few to be more than suggestive. It would seem that further experiments upon the relation of fatigue, and of the effective practice got during each experiment, to previous training, etc., might be quite varied in direction and have some educational interest; the best training, *cœteris paribus*, being presumably that which best enables the trained to utilize fresh opportunities for training of a kind somewhat new to him.

2. Tho probable error of an estimated distance is, of course, some function of the distance and of other data; but *what* function of the distance, when the other data remain, as far as may be, constant? May it not be commonly taken as some low power of the distance whose exponent increases slowly with the distance? In the present case the ratio of the two distances tried is 4.37 : 1; and the average odserver's mean deviation in inches from the truth, and from his own average estimate, respectively, are 2.69 and 2 56 times greater for the long distance than for the short; so that the exponent here would not be far from $\frac{2}{3}$.

J. E. OLIVER.

Ithaca, N.Y., March 5.

Work and its Relation to Gaseous Compression and Expansion.

IT is quite well known that the fundamental, and perhaps the most important hypothesis in theoretical meteorology is this, that work is done by air in expanding, and that heat is evolved whenever air is compressed. See "Recent Advances in Meteorology," p. 41. There is a most serious fallacy in this theory, however, in that it ignores the resistance against which the air expands, and considers that the mere diminution of the distance of the molecules of a gas, without the direct expenditure of external energy in changing this distance, can evolve heat.

An illustration will serve to make this clear. Take a cylinder one square foot in area and two feet high with a piston at the top and the air beneath it at atmospheric pressure. Place weights, pound by pound upon the piston, allowing all the heat developed to escape into the outside air. When we have added 2,160 pounds, the air beneath will be compressed to two atmospheres. Fasten the piston and its load, and connect the cylinder with another holding one cubic foot and containing air at normal pressure. An equilibrium will be quickly established and the pressure will be at 1.5 atmospheres in each cylinder. The potential energy remains the same as before; no work has been done and therefore there has been no change in temperature, except a slight chilling and heating due to the rush of the air from one into the other.

Return to the cylinder with the air compressed to two atmospheres and having the same temperature as the outside air. Take off the weight from the piston pound by pound, and the air will gradually expand, and in doing so will lift a weight, thereby doing work which cools the air very greatly, about 50° F. if the initial temperature was 60°. Instead of taking off the weight pound by pound, however, suppose the whole 2,160 pounds had been removed instantly. The only resistance which kept the air compressed has been entirely removed, and it is very evident that the air would expand without doing any work, if we consider that the piston moves back slowly; or, in other words, if we neglect the resistance of the air to the rapid motion of the piston, and hence there would be only a very slight chilling, owing to the work of imparting a certain velocity to the particles rushing out. The same result would have been attained if we had fastened the piston and its load, and then had turned a stop-cock, allowing the air to escape into the atmosphere without making a noise.

I am well aware that the ordinary interpretation of this illustration is very different; for example, Tyndall, in his "Heat as a Mode of Motion," p. 64, in a somewhat similar discussion, says: "The gas, in this experiment, executes work. In expanding it has to overcome the downward pressure of the atmosphere, which amounts to 15 pounds on every square inch, and also the weight on the piston itself. It is just the same as what it would accomplish if the air in the upper part of the cylinder were entirely abolished, and the piston had a weight of 4,320 pounds." I do not see that this changes the aspect of the case at all. Suppose that the air were compressed to two atmospheres beneath the piston, and that that was loaded with 4,320 pounds, while a perfect vacuum existed in the upper part of the cylinder, suppose that we suddenly remove 2,160 pounds from the piston. The piston, still having a load of 2,160 pounds, would fly to the top of the cylinder. How much work has the air done in expanding from two atmospheres to one? None at all. It looks very much as though the compressed air must have lifted that weight, but a little reflection will show that this is not the case. The best way to understand it, perhaps, would be to think of the weight after it reached within .001 of an inch of the top of the cylinder. Here is a weight of 2,160 pounds with the air under it at atmospheric pressure; in one sense the air sustains the weight, but if the air at atmospheric pressure sustains the weight at this point (the top of the cylinder), then the air at the same pressure would have sustained it at the middle of the cylinder. In other words, if we had allowed the compressed air to escape when the piston was at the centre of the cylinder, still with its load of 2,160 pounds and with a perfect vacuum above, there would have been an equilibrium, and we could have pushed the weight up and down, allowing it to stand at any point so long as the outside air had a communication with the lower side of the piston. Does not all this show that the compressed air, considered by itself, did not support any part of the weight at the middle of the cylinder, but was free to expand without lifting any weight or doing any work?

We are strictly taught that the old idea, "nature abhors a vacuum," is not at all tenable; but if we lay aside strict analysis for a moment and resort to this view, I think it will make the situation plainer to us. To all intents and purposes, when our piston loaded with 2,160 pounds had a perfect vacuum above it, we may say that it was sustained by that vacuum, or, at least, that the compressed air had nothing to do in supporting it or in moving it to the top of the vacuum. This seems to be quite an intricate problem, but a little reflection will show that the piston loaded to 2,160 pounds, and having a perfect vacuum above it, with air having free access to its under side, is in precisely the condition it would be in if both ends of the cylinder were open to the air and the piston without weight were located at any point in the cylinder. In this case the piston may be pushed up and down without meeting any resistance except that to the flow of the air.

Consider now the question of heated air rising in the atmosphere. We may simplify the problem slightly by taking a balloon, having an infinitely flexible envelope and without weight. Empty the balloon, and tie the neck so that no air can enter. It would require a pull of 15 pounds to the square inch to separate the sides of the balloon, owing to the pressure of the air. Incredible as it may seem, this is the force which theoretical meteorology has introduced into every discussion of the dynamical heating and cooling of the air, and of the cooling and heating of masses of air as they ascend or descend in the atmosphere,—a force which it is no exaggeration to say is at least 25,000 times as great as that really exerted or developed. Inflate the balloon one-third full with hydrogen gas. The work required to do this is that needed to displace a volume of air equal to the volume of gas which enters the balloon, or it would be that of lifting a weight equal to 1.2 ounces per cubic foot half the height of the balloon. It will probably be said that the outside air helps in this inflation, and I grant that for argument's sake.

Let the neck of the balloon remain open to the outside air, and suppose that the gas can just lift a weight attached to the balloon. The balloon will rise in the atmosphere to a point where the pressure is 10″, or until the gas has expanded to fill the whole envelope. Since the work of the balloon is open to the air, the pressure inside will continue exactly the same as that outside. A little reflection will show, however, that the conditions would be